PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Discrepancies in Self-Reported Financial Conflicts-of-Interest Disclosures by Physicians: Systematic Review
AUTHORS	Taheri, Cameron; Kirubarajan, Abirami; Li, Xinglin; Lam, Andrew; Taheri, Sam; Olivieri, Nancy

VERSION 1 – REVIEW

REVIEWER	Lisa Bero The University of Colorado United States
REVIEW RETURNED	17-Nov-2020

GENERAL COMMENTS	This is a novel review of studies that have compared physician conflicts of interest disclosed in documents (such as scientific papers, guidelines) with data available on payments to physicians. I think it is important to pull this information together so that readers are aware of the scope of the problem. I have a number of suggestions for improving the methods. Introduction Para 2 presents conflicting information. The authors state: “The Institute of Medicine, a US non-profit organization which is independent government and which provides policy recommendations for public health and science, asserts that disclosures of conflicts of interest protect the integrity of professional judgment and preserve the public trust in physicians.[9] As such, over the past decade, many academic institutions and medical journals have adopted guidelines which guide disclosures of financial COI in a putative effort to increase transparency and encourage critical appraisal of research findings.” The above is a bit contradictory because COI disclosure does increase transparency, but there is no evidence that disclosure protects the integrity of professional judgement. I would rephrase to suggest the disclosure is critical for transparency, to encourage critical appraisal, and to enable research into the effects of COI, but disclosure does not eliminate bias. Para 3, The review question stated as “systematic search of the literature on the discrepancies between actual and disclosed financial COI.” Please clarify that the objective of the review is to identify studies, not discrepancies Methods

	Overall, the methods need more explanatory detail. Para 1, page 5 states, “We included studies that sought to examine discrepancies between financial COI which were reported by physicians, and the objective data which documented payments from industry to physicians.” Please clarify if an eligible study could have had the examination of discrepancies as a primary or secondary objective. In my reading of this literature, many studies seem to have the identification of discrepancies as a secondary objective, so I am assuming these are included. In addition, please list the types of “objective data” that were eligible for inclusion. Before creation of physician payment databases in many countries, a number of studies compared disclosures in one type of document (eg, a guideline) to disclosures in another type of documents (eg, peer-reviewed publications) over the same time period to identify discrepancies (eg, Moynihan Moynihan R, Lai A, Jarvis H, Duggan G, Goodrick S, Beller E, Bero L. Undisclosed financial ties between guideline writers and pharmaceutical companies: a cross-sectional study across 10 disease categories. BMJ Open 2019;0:e025864. doi:10.1136/ bmjopen-2018-025864) I understand that these studies may not be considered to have an “objective” comparison, but the reason for excluding these should be explained more clearly in the methods. Page 5, line 10 states that “only original, peer-reviewed literature in the English language” were included. Please give some indication of the types of study designs eligible for inclusion. Were research letters, analysis pieces included? The Joanna Briggs risk of bias tool for prevalence data (page 6, line 2) does not seem the best choice for these studies as most of them were cross-sectional in design. There may not be a risk of bias tool suitable for these studies, but the authors could report on specific characteristics associated with bias, such as validity of the “objective’ comparator, characteristics of the sample. Page 6, line 13 states: ““a meta-analysis of the studies which reported the data necessary to compute the proportion of payments discrepant and the amount of funds discrepant.” My main concern with the methods is that I do not think that a meta-analytic summary is appropriate given the heterogeneity of the data. The populations are not comparable (ie, a set of guidelines, meeting abstracts, scientific publications), so the proportions of discrepancies will not be comparable. I would suggest reporting the range of proportions of discrepancies by document type. Results These studies are difficult to identify through automated searching, so I am not surprised the authors had to screen almost 6000 studies. Good work! Page 8, line 17. As noted above, although the authors pooled data by articles, authors, disclosure statements and self-reports, these data are still to heterogeneous to combine. An I2 of 94-99% is extremely high heterogeneity. Furthermore, these categories do not match the prespecified groupings of authorship, author, article or payment as defined in Table 1. Page 12, line 21. If only 9 of 40 studies reported the proportion of

	relevant discrepancies, what were the other studies reporting? If they were counting any financial ties, then it is not surprising there was a mismatch with guidelines or publications as these usually ask authors to disclose relevant financial ties. Or was this a problem with how the included papers defined a discrepancy? The issue that a large proportion of studies did not assess relevant financial ties should be discussed as a limitation. Page 12, line 31. Heterogeneity is also too high to pool proportion of funds. There is interesting data from the 15 studies that examined factors associated with discrepant reporting. This section would benefit from qualitative analysis by themes or factors. Right now the data are reported primarily as counts (how many studies examined the factor, and how many found an association). Discussion The finding that physician self-report of financial COI is discrepant with other datasources is supported by the data in the paper. Trying to identify a financial tie as “relevant” provides a real loophole in disclosure policies, and could be one reason some of the included studies did not attempt to assess relevance. I suggest adding a recommendation / solution of eliminating the judgement of relevance on disclosure forms as other have done (eg, Dunn, nature 2016). The authors provide some suggestions for improving the disclosure process (page 22, line 8-20), but these should be put in context of recent recommendations to improve disclosure through enforced, structured reporting and a process to assess relevance (eg, Grundy, BMJ 2020;368:m422). In addition, the abstract and discussion focus on recommendations for journals, but the authors should consider how these might apply to guideline development organizations. References The references need major editing and updating. For example, refs 1 and 7, 5 and 9 are duplicates. References 6 (2003) had been superseded by an updated Cochrane review (Lundh, 2017). Some recent studies on improving disclosure practices have not been cited.

REVIEWER	Nina Kreuzberger University Hospital Cologne
REVIEW RETURNED	01-Dec-2020

GENERAL COMMENTS	This paper examines the available literature on reporting of financial conflicts of interests of physicians resulting from payments by the industry. The topic is interesting and a general problem in publications and clinical guidelines, and especially difficult to examine in countries without a public database for registration of financial flows between industry and physicians. With some changes, I’d recommend publishing this systematic review. I could not find the appendix with the search strategy, please provide this for the final publication. It may be sufficient to reference the PRISMA statement, I don’t think it is necessary as appendix. Methods Why did you exclude presentations and abstracts? Please provide the rationale in your publication. p. 6 l. 26: “Subject-specific search terms adapted from previously

	published systematic reviews on financial COI (“conflict of interest”, “financial support”, and “funding”)” – Could you cite these reviews? Please provide some more details how you did your analysis. Results Could you sort the studies according to their groups (authorships, authors, articles, payments) in the various tables, or add this information in table 2? p. 8, l. 49: studies don’t add up, as the studies using both sources are already listed as OPD p. 8 l. 50: Are both references 12 and 46 included? p. 9 l. 13: “The majority of studies defined discrepancies as one or more undisclosed COI, but some studies used alternative definitions.” Could you be more specific? Is this per author, or per article, or per authorship? p. 9 l. 18: Please refer to the respective meta-analysis that you are mentioning here, and make them the same size as they are very small (at least in the download I saw). Please also add the number of studies and number of examined articles/authors/disclosure statements/payments per pooled result. p. 9 l. 24: Did you do subgroup analysis to examine heterogeneity? If not, provide a reason why. As heterogeneity is so high, do you think you should pool the results at all? p. 13 l. 21 Please reference the studies p. 13 l. 33: “The pooled proportion of total payment amounts which were discrepant was 33%. Heterogeneity between studies was high I2=100%” – again, please provide more details: add the study references, or refer to a figure where they are clearly listed. If you pool a result, please provide 95% Cis consistently. Please add the number of payments considered. Is pooling appropriate here? p. 14, l. 15: Maybe you can combine some things to make the paragraph shorter and easier to read (also for the next few paragraphs of listing factors). Table 3, at first reading, “words per second” is confusing – maybe add somewhere that it is a presentation? p.14 l. 39 “Three of these studies found a positive association …” – just an association, as with specialties, there should be no directions p. 18 l. 50: “All studies had a low risk of bias overall.” – please be careful with overall risk of bias judgements, as bias in one domain is already a bias. Risk of bias: Could you please provide your decision rule regarding the cut-off for item 3? When did you consider sample size adequate? p. 9 l. 15: “Three studies considered a discrepancy to occur only when all COI were inaccurately disclosed by an author.[36, 47, 54]” – these studies are together in one meta-analysis with all other studies. Seen their different definition of discrepancy, this may not be appropriate. Please reference this main analysis at the respective part in the text. Discussion p. 21 l. 36: “The most common explanation for failure …” – please make clear where this result comes from – this was only one study (correct?) that is used to explain pooled results that are extremely heterogeneous. Please add the number of studies/entities examined for the results. p. 23 l. 8 “The findings of this systematic review and meta-analysis suggest that changes to COI disclosure policies are required in the interests of transparency, otherwise self-reported disclosure will

	continue to remain an empty panacea." – before, you said that COI disclosure policies had no effect on COI reporting, so be more specific here already. p. 23, l. 11: "One possible solution is for journals to provide authors with prepopulated disclosure forms with data extrapolated from public databases" – these don't exist in many countries, how would you deal with these? Could you address publication bias? Language/Readability Please have a look at sentence structure and repetition, you may be able to move together and shorten some passages, which would facilitate reading the paper (e.g. defining objective payment directly after you bring it up the first time in eligibility criteria). Below you find a few smaller language/grammar things I noticed: p. 5 l.23: "a US non-profit organization which is independent government ---" – Please check the readability of this sentence. p. 5 l. 45: "Our study aims to systematically examine the completeness of self-reported financial COI disclosures by physicians, and identify the factors associated with non-disclosure." – systematically examine the literature on completeness of … p. 6 a priori. Remove the point p. 6 l.49 From each study, we extracted the clinical focus, study design, primary objective, sources of data collection, time period during, how COI were defined, number and monetary amount of total COI, number and monetary amount of undisclosed COI, number of relevant undisclosed COI, types of undisclosed COI, factors associated with undisclosed COI, reasons for non-disclosure, and association of nondisclosure with positive study outcomes.– please check readability p. 7 l. 13: qualitative synthesis – in my understanding, qualitative synthesis comes with a specific framework of interpreting data. I would swap words and say summarized as narrative or similar. p.9 l.11 "As above the included studies examined COI involving articles, authors, disclosure statements, or payments." – as outlined above? p. 15 l. 9 "Payments that were provided but were unrelated to the topic of a presentation or article in which the authors failed to disclose were more likely to be undisclosed when compared to directly and/or indirectly related payments" – simplify, e.g. "Payments that were unrelated to the topic of presentation or article were more likely to be undisclosed than directly or indirectly related payments." p. 15 l. 18 "Likewise, commentaries were significantly less likely to have adequate disclosure compared to studies with original data." You can move the reference to the sentence above, as previously somewhere you say that commentaries are a level of evidence. p.18 l. 12 "Reported explanation for discrepant reporting of fCOI" – f too much p. 18 l. 29 "Data concerning the association unreported COI and the outcome of the research was reported by three studies, but the results are conflicting." - structure p. 18 l. 44 "Several studies did not use a wide-enough sample frame to address the study's target population." Please add the references p. 21 l. 38: "But in fact, a median of 45% of the non-disclosed payments from pharmaceutical companies or device manufacturers were directly or indirectly involved in the published or presented academic work." – was the payment involved? Or supporting the work? If not, restructure the sentence p. 21 l. 46 "cStrengths of our review include the robust search

	strategy" - remove the c p. 22 l. 41: Despite efforts to standardize the disclosure process, physicians many continue to omit reporting relevant disclosures due to false convictions that their relationships with industry do not apply to their work" – sentence structure p. 22 l. 45 "Our analysis found, however, that a …" was it your analysis or the primary study?

REVIEWER	Oscar Olavarria, MD MS McGovern Medical School at UTHealth (Houston, TX, USA)
REVIEW RETURNED	13-Dec-2020

GENERAL COMMENTS	The authors are to be congratulated for a thorough systematic review and meta-analysis of the medical literature related to conflicts of interest (COI) disclosed by physicians. The manuscript is clear and well-written and reflects a rigorous statistical analysis of highly selected data. In general, I felt that your Methods were well-described and thorough, and that you captured many of the concerns about the importance of COI in medical publications. Although I believe that the results of this study do not necessarily have the potential to change practice, it highlights the high prevalence of discordance in reporting of COI. Please consider the following comments: -The methods section in abstract does not reflect your study design or methodology. Consider including in a table the definition of COI employed by each of the included studies. For example: did all studies consider food and beverage as COI? Was there a certain amount in dollars below which a payment was not considered a COI? Although I agree that transparency in reporting is key, I question the significance of small expenses (e.g. food and beverage) to "interfere the proper exercise of judgement". Should authors be in the moral obligation of reporting food and beverage expenses? Is there a certain limit over which these should be considered a true conflict of interest? Are these expenses of equivalent importance as payments for consulting or honoraria? Did you assess for potential publication bias when performing meta-analysis? Although not supported by evidence, it is reasonable to imagine that studies showing discordance in reporting of COI are more likely to be published than a study showing no discordance in reporting. Therefore the importance of assessing potential publication bias. Moreover, reviewers and editors with COI can block publication of articles with results contrary to their interests. In order to increase transparency, should reviewers and journal editors disclose their COI when reviewing manuscripts? Considering the important heterogeneity in methodologies of the included studies as well as considerably high I2 is a meta-analysis valid? Consider deleting meta-analysis and present results of qualitative synthesis only. Figure 1 (PRISMA flow diagram). Numbers do not add up in box with full articles excluded

REVIEWER	Abdelrahman I. Abushouk Cleveland Clinic Foundation, Cleveland, Ohio
REVIEW RETURNED	04-Jan-2021

GENERAL COMMENTS	The authors performed a comprehensive systematic review on the disclosure of conflicts of interests (COI) by physicians in academic

	publications and articles. They showed a high level of discrepancy between objective data sources and self-reported COI by physicians. Their analytical approach (qualitative and quantitative) is appropriate and their discussion of the findings is to the point. I have some recommendations. * Abstract - The abstract results should include the results of quantitative meta-analysis, adding 95% CI and p values if available. - It might also be valuable to add the factors related to non-disclosure from qualitative analysis. * Introduction - "The Institute of Medicine" I believe the name was changed many years ago into the "National Academy of Medicine". - Line 29: Can the authors cite some of these guidelines? * Methods - I believe the authors should use the MOOSE checklist, which is more fitting to reviews of observational studies; unlike PRISMA (Interventional). - Page 5, Line 15 "We considered objective payment data to be any data that was not reported by physicians themselves": This is probably a major cause of the extreme heterogeneity observed in most outcomes. This might work as a culprit of subgroup analysis to address such heterogeneity. - Page 5, Line 26: Please add the citations for some of the source systematic reviews. - Most readers are not familiar with the Joanna Briggs Checklist. Please introduce it in your methods section and mention how individual studies are scored. - The study outcomes are well-defined in the methodology section. I applaud the authors' work in this regard. - The authors should indicate that they used the random-effects model because of the expected methodological heterogeneity between the studies, regardless the I2 value. - Did the authors assess the risk of publication bias (Panel D contains more than 10 studies)? - Further, if possible, a GRADE assessment would be useful to draw recommendations for future research in this field. * Results - Page 8, line 23: Did the authors attempt any method to address such high heterogeneity observed in all meta-analysis outcomes? - Page 8, Line 24: How did the authors assess the discrepancy rates over time? Did they create time intervals for different studies? - Page 12, line 33: 33% (confidence interval?) - Page 17, line 43: Should be "Table 4". Also, I believe this is more suited as a figure rather than a table. - How was a study judged to be of an OVERALL low risk of bias? - The qualitative analysis of "Factors associated with discrepant reporting" is well-presented. * Discussion - Please provide an adequate explanation for the significant heterogeneity observed. - The authors make recommendations for journal editors. However, I believe more recommendations should be addressed for academic institutions as well to educate their researchers on adequate disclosure of COI and put stronger policies to address non-

	compliance to such guidelines. - Also, recommendations for future research are valuable. * General There are several linguistic and grammatical errors in the manuscript. e.g. Page 3, line 29 "Physicians self-report of financial COI are highly", page 5, line 51 "were defined number and", etc.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

1. Para 2 presents conflicting information. The authors state: “The Institute of Medicine, a US non-profit organization which is independent government and which provides policy recommendations for public health and science, asserts that disclosures of conflicts of interest protect the integrity of professional judgment and preserve the public trust in physicians.[9] As such, over the past decade, many academic institutions and medical journals have adopted guidelines which guide disclosures of financial COI in a putative effort to increase transparency and encourage critical appraisal of research findings.” The above is a bit contradictory because COI disclosure does increase transparency, but there is no evidence that disclosure protects the integrity of professional judgement. I would rephrase to suggest the disclosure is critical for transparency, to encourage critical appraisal, and to enable research into the effects of COI, but disclosure does not eliminate bias.

We agree with the reviewer and have rephrased parts of paragraph 2 to make clear that disclosure does not eliminate bias.

2. Para 3, The review question stated as “systematic search of the literature on the discrepancies between actual and disclosed financial COI.” Please clarify that the objective of the review is to identify studies, not discrepancies

We thank the reviewer for their comment and have clarified that the objective of the review is to identify studies, not discrepancies.

3. Overall, the methods need more explanatory detail. Para 1, page 5 states, “We included studies that sought to examine discrepancies between financial COI which were reported by physicians, and the objective data which documented payments from industry to physicians.” Please clarify if an eligible study could have had the examination of discrepancies as a primary or secondary objective. In my reading of this literature, many studies seem to have the identification of discrepancies as a secondary objective, so I am assuming these are included.

We have clarified that an eligible study could have had the examination of discrepancies as either a primary or secondary objective.

4. In addition, please list the types of “objective data” that were eligible for inclusion. Before creation of physician payment databases in many countries, a number of studies compared disclosures in one type of document (eg, a guideline) to disclosures in another type of documents (eg, peer-reviewed publications) over the same time period to identify discrepancies (eg, Moynihan Moynihan R, Lai A, Jarvis H, Duggan G, Goodrick S, Beller E, Bero L. Undisclosed financial ties between guideline writers and pharmaceutical companies: a cross-sectional study across 10 disease categories. BMJ Open 2019;0:e025864. doi:10.1136/ bmjopen-2018-025864) I understand that these studies may not be considered to have an “objective” comparison, but the reason for excluding these should be

explained more clearly in the methods.

Thank you for your feedback. We have clarified that an objective comparison is any source that is not self-reported by a physician in order to get the most complete estimate of disclosure. We have also clarified in our Methods under “Eligibility criteria” that comparisons between self-reported disclosures would not be eligible for our study, though, we acknowledge that this is a potential area of future study as it is quite interesting.

5. Page 5, line 10 states that “only original, peer-reviewed literature in the English language” were included. Please give some indication of the types of study designs eligible for inclusion. Were research letters, analysis pieces included?

We included all original observational study designs, including cross-sectional analyses, prospective cohorts, and retrospective cohorts. Research letters, analysis pieces, editorials, and abstracts were not eligible for inclusion as only full-text manuscripts were eligible. We have clarified our Methods section to make this more clear.

6. The Joanna Briggs risk of bias tool for prevalence data (page 6, line 2) does not seem the best choice for these studies as most of them were cross-sectional in design. There may not be a risk of bias tool suitable for these studies, but the authors could report on specific characteristics associated with bias, such as validity of the “objective’ comparator, characteristics of the sample.

We agree with the reviewer that there is no existing risk of bias tool that was designed with these studies in mind. As such, we have modified the Joanna Briggs tool to include the domains of relevance. Furthermore, we agree that it is important to report on the validity of the “objective” comparator. Question 6 of our modified Joanna Briggs tool (Were valid methods used for the identification of the condition?) addresses this particular characteristic for each included study. We have clarified that this question is referring to the objective payment data at the bottom of Figure 2.

7. Page 6, line 13 states: ““a meta-analysis of the studies which reported the data necessary to compute the proportion of payments discrepant and the amount of funds discrepant.” My main concern with the methods is that I do not think that a meta-analytic summary is appropriate given the heterogeneity of the data. The populations are not comparable (ie, a set of guidelines, meeting abstracts, scientific publications), so the proportions of discrepancies will not be comparable. I would suggest reporting the range of proportions of discrepancies by document type.

We thank the reviewer for their insightful comment. The high heterogeneity precludes the meaningful interpretation of the pooled results. In response, we have refocused our article primarily on the qualitative synthesis and have relegated the meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We still believe in presenting the meta-analysis in the Appendix in order to visually illustrate the range of discrepancies and the heterogeneity between studies. However, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the reasons you have mentioned. Furthermore, we have included the range of proportion of discrepancies at each of the four levels (article, payment, disclosure statement, and author) in our results.

8. These studies are difficult to identify through automated searching, so I am not surprised the authors had to screen almost 6000 studies. Good work!

Many thanks!

9. Page 8, line 17. As noted above, although the authors pooled data by articles, authors, disclosure

statements and self-reports, these data are still to heterogeneous to combine. An I2 of 94-99% is extremely high heterogeneity. Furthermore, these categories do not match the prespecified groupings of authorship, author, article or payment as defined in Table 1.

We have amended the prespecified groupings mentioned in the text to match those defined in Table 1.

10. Page 12, line 21. If only 9 of 40 studies reported the proportion of relevant discrepancies, what were the other studies reporting? If they were counting any financial ties, then it is not surprising there was a mismatch with guidelines or publications as these usually ask authors to disclose relevant financial ties. Or was this a problem with how the included papers defined a discrepancy? The issue that a large proportion of studies did not assess relevant financial ties should be discussed as a limitation.

We understand the reviewer's concern regarding the low proportion of studies assessing relevant discrepancies however our study suggests that physicians are poor assessors of payment relevance. Nonetheless, we respect this perspective and have added a line to clarify this at the end "Strengths and Weaknesses".

11. Page 12, line 31. Heterogeneity is also too high to pool proportion of funds.

We are in agreement again with the reviewer. The high heterogeneity precludes the meaningful interpretation of the pooled results. Similar to our COI discrepant analysis, we will refocus our article primarily on the qualitative synthesis and the meta-analyses will just be exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We have included the range of proportion of funds discrepant in our results. Again, we believe in presenting the meta-analysis in the Appendix in order to visually illustrate the range of discrepancies and the heterogeneity between studies. However, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the reasons you have mentioned.

12. There is interesting data from the 15 studies that examined factors associated with discrepant reporting. This section would benefit from qualitative analysis by themes or factors. Right now the data are reported primarily as counts (how many studies examined the factor, and how many found an association).

We thank the reviewer for this comment. We have re-organized our qualitative synthesis to make more clear our four themes: factors related to author characteristics (e.g., academic affiliation), payment characteristics (e.g., amount of the payment from industry), article characteristics (e.g., level/hierarchy of evidence, such as systematic review versus commentary), and journal characteristics (e.g., impact factor). We have also combined and revised parts of this section ("Factors associated with discrepant reporting") to increase readability.

13. The finding that physician self-report of financial COI is discrepant with other datasources is supported by the data in the paper. Trying to identify a financial tie as "relevant" provides a real loophole in disclosure policies, and could be one reason some of the included studies did not attempt to assess relevance. I suggest adding a recommendation / solution of eliminating the judgement of relevance on disclosure forms as other have done (eg, Dunn, nature 2016).

We agree with the reviewer and have added this recommendation to the section on "Meaning of the study".

14. The authors provide some suggestions for improving the disclosure process (page 22, line 8-20),

but these should be put in context of recent recommendations to improve disclosure through enforced, structured reporting and a process to assess relevance (eg, Grundy, BMJ 2020;368:m422). In addition, the abstract and discussion focus on recommendations for journals, but the authors should consider how these might apply to guideline development organizations.

We have added a statement to put our suggestions for improving the disclosure process in the context of recent recommendations in the section on “Meaning of the study”. We have also clarified that these recommendations can apply to both journals, guidelines development organizations, and academic institutions.

15. The references need major editing and updating. For example, refs 1 and 7, 5 and 9 are duplicates. References 6 (2003) had been superseded by an updated Cochrane review (Lundh, 2017). Some recent studies on improving disclosure practices have not been cited.

We have edited the references to remove duplicates and updated reference 6. We have also added recent studies on improving disclosure practices including Grundy 2020.

Reviewer 2

1. I could not find the appendix with the search strategy, please provide this for the final publication. It may be sufficient to reference the PRISMA statement, I don’t think it is necessary as appendix.

Thank you for this comment. We have provided the search strategy in case readers find it of interest.

2. Why did you exclude presentations and abstracts? Please provide the rationale in your publication.

We thank the reviewer for their comment. We excluded presentations and abstracts as most presentations/abstracts are not transparent on whether they are peer-reviewed, and we required a more complete picture of data in order to run our meta-analysis. Many presentations and abstracts contained missing information due to the constraints of the word limits, and therefore were not appropriate for inclusion. We have provided this rationale in the methods section of our manuscript under “Eligibility criteria”.

3. p. 6 l. 26: “Subject-specific search terms adapted from previously published systematic reviews on financial COI (“conflict of interest”, “financial support”, and “funding”)” – Could you cite these reviews?

We have cited the reviews from which we adapted subject-specific search terms.

4. Please provide some more details on how you did your analysis.

We have provided additional details on how we did our analysis in the last paragraph in the section “Statistical Analyses and Outcomes”.

5. Could you sort the studies according to their groups (authorships, authors, articles, payments) in the various tables, or add this information in table 2?

We thank the reviewer for their suggestion. We have added this information to Table 2 as they have requested.

6. p. 8, l. 49: studies don’t add up, as the studies using both sources are already listed as OPD

Thank you for your comment, this has been amended.

7. p. 8 l. 50: Are both references 12 and 46 included?

References 12 and 46 referenced the web pages of device manufacturers.

8. p. 9 l. 13: “The majority of studies defined discrepancies as one or more undisclosed COI, but some studies used alternative definitions.” Could you be more specific? Is this per author, or per article, or per authorship?

We have clarified what the alternative definition is by some studies. The pre-specified groupings in Table 1 demonstrate how the definition is applied to authors, articles, and authorships.

9. p. 9 l. 18: Please refer to the respective meta-analysis that you are mentioning here, and make them the same size as they are very small (at least in the download I saw). Please also add the number of studies and number of examined articles/authors/disclosure statements/payments per pooled result.

Per your latter suggestion, we have removed the meta-analyses as a primary Figure and have made the meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We have improved the visual quality and size of the Appendix figure. Furthermore, we have included the number of studies and the number of examined articles/authors/disclosure statements/payments, and funds analyzed, per pooled result.

10. p. 9 l. 24: Did you do subgroup analysis to examine heterogeneity? If not, provide a reason why. As heterogeneity is so high, do you think you should pool the results at all?

We are in agreement with the reviewer. The high heterogeneity precludes the meaningful interpretation of the pooled results. We will refocus our article primarily on the qualitative synthesis and have relegated meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We believe in presenting the meta-analysis in the Appendix in order to visually illustrate the range of discrepancies and the heterogeneity between studies. This serves to emphasize why the pooled results should be interpreted with caution. Furthermore, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the reasons you have mentioned here and below.

11. p. 13 l. 21 Please reference the studies

We have referenced the studies which reported the proportion of relevant discrepancies.

12. p. 13 l. 33: “The pooled proportion of total payment amounts which were discrepant was 33%. Heterogeneity between studies was high I2=100%” – again, please provide more details: add the study references, or refer to a figure where they are clearly listed. If you pool a result, please provide 95% Cis consistently. Please add the number of payments considered. Is pooling appropriate here?

Again, we are in agreement with the reviewer. The high heterogeneity precludes the meaningful interpretation of the pooled results. The meta-analyses are now labeled as exploratory and the forest plots are no longer part of the primary figures; they have been moved to the appendix. We have included: 1) the number of studies 2) the total funds analyzed for the pooled result 3) the location of this result (Appendix) 4) a consistent method of reporting CI’s and ranges.

13. p. 14, l. 15: Maybe you can combine some things to make the paragraph shorter and easier to read (also for the next few paragraphs of listing factors).

We thank the reviewer for their comment. We have revised these paragraphs and combined sentences to increase readability.

14. Table 3, at first reading, “words per second” is confusing – maybe add somewhere that it is a presentation?

We have clarified that this is with respect to a presentation.

15. p.14 l. 39 “Three of these studies found a positive association …” – just an association, as with specialties, there should be no directions

We have removed the word positive.

16. p. 18 l. 50: “All studies had a low risk of bias overall.” – please be careful with overall risk of bias judgements, as bias in one domain is already a bias.

We agree with the reviewer and have removed the overall risk of bias judgment.

17. Risk of bias: Could you please provide your decision rule regarding the cut-off for item 3? When did you consider sample size adequate?

We considered a study to have an adequate sample if their sample was consistent with an appropriate sample size calculation. In the absence of a sample size calculation, we considered a sample greater than 1000 to be appropriate. We have added this decision rule to our methods under “Data collection”.

18. p. 9 l. 15: “Three studies considered a discrepancy to occur only when all COI were inaccurately disclosed by an author.[36, 47, 54]” – these studies are together in one meta-analysis with all other studies. Seen their different definition of discrepancy, this may not be appropriate. Please reference this main analysis at the respective part in the text.

We are again in agreement with the reviewer. The high heterogeneity, stemming from different definitions of discrepancies, precludes the meaningful interpretation of the pooled results. In response, we have refocused our article primarily on the qualitative synthesis and have relegated meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We have cited this specific example in the limitation section of our discussion where we outlined that the threshold for an author to be labelled as discrepant varied between studies.

19. p. 21 l. 36: “The most common explanation for failure …” – please make clear where this result comes from – this was only one study (correct?) that is used to explain pooled results that are extremely heterogeneous. Please add the number of studies/entities examined for the results.

We have clarified that this result comes from one study. Furthermore, we have included the number of studies and the number of examined articles/authors/disclosure statements/payments, and funds analyzed, per pooled result.

20. p. 23 l. 8 “The findings of this systematic review and meta-analysis suggest that changes to COI disclosure policies are required in the interests of transparency, otherwise self-reported disclosure will continue to remain an empty panacea.” – before, you said that COI disclosure policies had no effect on COI reporting, so be more specific here already.

We have clarified that disclosure practices need to go beyond existing ICMJE policy recommendations. We have also outlined specific suggestions to enhance transparency.

21. p. 23, l. 11: “One possible solution is for journals to provide authors with prepopulated disclosure forms with data extrapolated from public databases” – these don’t exist in many countries, how would you deal with these?

We completely agree with the reviewer’s concern that this type of data does not exist in all countries. Our hope is that we can encourage other countries to adopt similar policies requiring industry disclosures. In the meantime, we have suggested that authors report all disclosures which can then be subsequently verified by an unbiased party to determine relevance.

22. Could you address publication bias?

We agree with Reviewer 2 that publication bias may be present. More specifically, studies that demonstrate a high discrepancy may be published in favour of studies with low discrepancies. However, the high heterogeneity found in our exploratory meta-analyses, precluded a meaningful quantitative analysis of publication bias. We have included this explanation in the discussion section and is certainly a point to keep in mind for our readers when interpreting results in this field .

23. Please have a look at sentence structure and repetition, you may be able to move together and shorten some passages, which would facilitate reading the paper (e.g. defining objective payment directly after you bring it up the first time in eligibility criteria).

We have looked at increasing the readability of the paper. We have moved the definition of objective payment data to where we first bring it up in eligibility criteria to facilitate reading.

14. Below you find a few smaller language/grammar things I noticed: p. 5 l.23: “a US non-profit organization which is independent government ---“ – Please check the readability of this sentence.

The sentence has been amended to increase readability.

15. p. 5 l. 45: “Our study aims to systematically examine the completeness of self-reported financial COI disclosures by physicians, and identify the factors associated with non-disclosure.” – systematically examine the literature on completeness of …

We have amended this sentence to reflect that we are systematically examining the literature.

16. p. 6 a priori. Remove the point

We have removed the point.

17. p. 6 l.49 From each study, we extracted the clinical focus, study design, primary objective, sources of data collection, time period during, how COI were defined, number and monetary amount of total COI, number and monetary amount of undisclosed COI, number of relevant undisclosed COI, types of undisclosed COI, factors associated with undisclosed COI, reasons for non-disclosure, and association of nondisclosure with positive study outcomes.– please check readability

We have amended this sentence to increase readability.

18. p. 7 l. 13: qualitative synthesis – in my understanding, qualitative synthesis comes with a specific

framework of interpreting data. I would swap words and say summarized as narrative or similar.

We agree with the reviewer. We have swapped “qualitative synthesis” with “narrative summary”.

19. p.9 l.11 “As above the included studies examined COI involving articles, authors, disclosure statements, or payments.” – as outlined above?

We have added the word “outlined”.

20. p. 15 l. 9 “Payments that were provided but were unrelated to the topic of a presentation or article in which the authors failed to disclose were more likely to be undisclosed when compared to directly and/or indirectly related payments” – simplify, e.g. “Payments that were unrelated to the topic of presentation or article were more likely to be undisclosed than directly or indirectly related payments.”

We thank the reviewer for their suggestion. We have simplified this sentence as they have suggested.

21. p. 15 l. 18 “Likewise, commentaries were significantly less likely to have adequate disclosure compared to studies with original data.” You can move the reference to the sentence above, as previously somewhere you say that commentaries are a level of evidence.

We have moved the reference to the sentence above.

22. p.18 l. 12 “Reported explanation for discrepant reporting of fCOI” – f too much

We have removed the “f”.

23. p. 18 l. 29 “Data concerning the association unreported COI and the outcome of the research was reported by three studies, but the results are conflicting.” - structure

We have amended the sentence to increase readability.

24. p. 18 l. 44 “Several studies did not use a wide-enough sample frame to address the study’s target population.” Please add the references

We have added the references.

25. p. 21 l. 38: “But in fact, a median of 45% of the non-disclosed payments from pharmaceutical companies or device manufacturers were directly or indirectly involved in the published or presented academic work.” – was the payment involved? Or supporting the work? If not, restructure the sentence

We have amended this sentence to reflect that the payments were related either directly or indirectly to the work.

26. p. 21 l. 46 “cStrengths of our review include the robust search strategy” - remove the c

The “c” has been removed.

27. p. 22 l. 41: Despite efforts to standardize the disclosure process, physicians many continue to omit reporting relevant disclosures due to false convictions that their relationships with industry do not apply to their work” – sentence structure

The sentence has been revised to increase readability.

28. p. 22 l. 45 “Our analysis found, however, that a …” was it your analysis or the primary study? This was a finding of our meta-analysis examining the proportion of relevant COI that was not disclosed. We have clarified this in the manuscript.

Reviewer 3

1. The methods section in abstract does not reflect your study design or methodology.

The outcomes analysed and details of the meta-analyses have been included in the abstract. The results section of the abstract now provides a comprehensive view of the meta-analyses, now considered an exploratory, including the confidence interviews ranges.

2. Consider including in a table the definition of COI employed by each of the included studies. For example: did all studies consider food and beverage as COI? Was there a certain amount in dollars below which a payment was not considered a COI?

We thank the reviewer for their comment. The definition of COI employed by each of the included studies varied. For example, not all studies considered food and beverage a COI, and the threshold above which a payment was considered a COI was not consistent. This explains the significant heterogeneity of our meta-analyses. As such, we do not believe that it would be useful for the reader to have a table of the specific definitions of COI per study. Nonetheless we respect this perspective, and have highlighted this as a limitation of our study in the discussion under “Strengths and weaknesses of the study”.

3. Although I agree that transparency in reporting is key, I question the significance of small expenses (e.g. food and beverage) to “interfere the proper exercise of judgement”. Should authors be in the moral obligation of reporting food and beverage expenses? Is there a certain limit over which these should be considered a true conflict of interest? Are these expenses of equivalent importance as payments for consulting or honoraria?

We understand the reviewer’s concern regarding the significance of small expenses. There is a common perception that small expenses such as food and beverage are unlikely to affect decision-making or behavior. However, the often-advanced idea that small payments from industry are unlikely to affect physician judgment in research or medical practice is not supported by the literature. By contrast it is clear that feelings of obligation and impulses toward reciprocity are not related to the size of a gift; small as well as larger gifts are associated with increased rates of prescribing brand-name medications. Nonetheless, we respect this perspective and have added it to our manuscript in the discussion.

4. Did you assess for potential publication bias when performing meta-analysis? Although not supported by evidence, it is reasonable to imagine that studies showing discordance in reporting of COI are more likely to be published than a study showing no discordance in reporting. Therefore the importance of assessing potential publication bias.

We agree with Reviewer 3 that publication bias may be present. Although there is no published evidence in this specific field point towards bias, it is not a stretch to imagine studies that demonstrate a high discrepancy may be published in favour of studies with low discrepancies. However, the high heterogeneity found in our exploratory meta-analyses precluded a meaningful quantitative analysis of publication bias. We have included this explanation in the discussion section and is certainly a point to keep in mind for our readers when interpreting results in this field.

5. Moreover, reviewers and editors with COI can block publication of articles with results contrary to their interests. In order to increase transparency, should reviewers and journal editors disclose their COI when reviewing manuscripts?

We thank the reviewer for their insightful comment. We agree that reviewers and journal editors should disclose their COI when reviewing manuscripts. However, as our study did not examine this population, we do not feel confident supporting these recommendations in this manuscript.

6. Considering the important heterogeneity in methodologies of the included studies as well as considerably high I2 is a meta-analysis valid? Consider deleting meta-analysis and present results of qualitative synthesis only.

We thank the reviewer for their insightful comment. We are in agreement that the high heterogeneity precludes the meaningful interpretation of the pooled results. In response, we have refocused our article primarily on the qualitative synthesis and have relegated meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We believe in presenting the meta-analysis in the Appendix in order to visually illustrate the range of discrepancies and the heterogeneity between studies. However, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the exact reasons you have mentioned.

7. Figure 1 (PRISMA flow diagram). Numbers do not add up in box with full articles excluded

We have amended the PRISMA flow diagram so that the numbers add up. The missing studies were excluded as there was no relevant outcome.

Reviewer 4

1. The abstract results should include the results of quantitative meta-analysis, adding 95% CI and p values if available.

We thank Reviewer 4 for their insightful comments. The results section of the abstract now provides a comprehensive view of the meta-analyses results, now considered an exploratory analysis, including the proportion of discrepancies across four levels of COI reporting and proportion of funds discrepant. Additionally, we have included confidence intervals and ranges for a complete picture of our analyses.

2. It might also be valuable to add the factors related to non-disclosure from qualitative analysis.

We have highlighted the most commonly reported factor related to non-disclosure from the qualitative analysis in the abstract.

3. "The Institute of Medicine" I believe the name was changed many years ago into the "National Academy of Medicine".

We thank the reviewer for catching this update to the name of the Institute of Medicine. We have revised the name in the manuscript.

4. Line 29: Can the authors cite some of these guidelines?

We have cited the ICMJE guidelines which are internationally recognized and commonly referenced.

5. I believe the authors should use the MOOSE checklist, which is more fitting to reviews of observational studies; unlike PRISMA (Interventional).

We thank the reviewer for their suggestion. This systematic review was conducted according to the standards and guidelines established by the Preferred Reporting Items for Systematic Reviews and Meta-Analysis (PRISMA) and the fourth edition of the Joanna Briggs Institute Reviewer's Manual. This was specified in advance and documented in our protocol. While we understand that the PRISMA guidelines were designed for interventional trials, the checklist can also be used as a basis for reporting systematic reviews of other types of research including observational studies. As such, BMJ Open has published other systematic reviews of observational studies that followed the PRISMA checklist. However, if the Editor believes that it is necessary to change to the MOOSE checklist, we are happy to adapt our manuscript to do so.

6. Page 5, Line 15 "We considered objective payment data to be any data that was not reported by physicians themselves": This is probably a major cause of the extreme heterogeneity observed in most outcomes. This might work as a culprit of subgroup analysis to address such heterogeneity.

We thank the reviewer for their insightful comment. We are in agreement that the varied definition of what constitutes discrepancy has led to high heterogeneity between studies. In response, we have refocused our article primarily on the qualitative synthesis and have relegated meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We believe in presenting the meta-analysis in the Appendix simply to visually illustrate the range of discrepancies and the heterogeneity between studies. However, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the exact reasons you have mentioned.

7. Page 5, Line 26: Please add the citations for some of the source systematic reviews.

Citations for the source systematic reviews have been added.

8. Most readers are not familiar with the Joanna Briggs Checklist. Please introduce it in your methods section and mention how individual studies are scored.

We have added a section to the methods section introducing the Joanna Briggs Checklist and have provided additional information regarding how individual studies are scored.

9. The study outcomes are well-defined in the methodology section. I applaud the authors' work in this regard.

Many thanks!

10. The authors should indicate that they used the random-effects model because of the expected methodological heterogeneity between the studies, regardless the I2 value.

Thank you for the suggestion, we have clarified our rationale for using a random-effects model in our methods.

11. Did the authors assess the risk of publication bias (Panel D contains more than 10 studies)?

We agree with Reviewer 4 that publication bias may be present. More specifically, studies that demonstrate a high discrepancy may be published in favour of studies with low discrepancies. However, the high heterogeneity found in our exploratory meta-analyses precluded a meaningful

quantitative analysis of publication bias. We have included this explanation in the discussion section and is certainly a point to keep in mind for our readers when interpreting results in this field .

12. Further, if possible, a GRADE assessment would be useful to draw recommendations for future research in this field.

We thank the reviewer for their suggestion. The intended purpose of our study was to determine the completeness of self-reported disclosures and determine the factors associated with non-disclosure. From this data, we developed recommendations for stronger well-enforced policies which can serve as a basis for future research. At this time we do not believe our suggestions have enough evidence to create a GRADE assessment. Nevertheless we respect the reviewer's perspective and have added recommendations for future research to investigate the effectiveness of various COI disclosure policies.

13. Page 8, line 23: Did the authors attempt any method to address such high heterogeneity observed in all meta-analysis outcomes?

Based on your previous comment, we have come to the conclusion that the high heterogeneity precludes the meaningful interpretation of the pooled results. In response, we have refocused our article primarily on the qualitative synthesis and have relegated the meta-analyses as exploratory. As such, the forest plots are no longer a primary figure but included in the appendix for completeness. We still believe in presenting the meta-analysis in the Appendix in order to visually illustrate the range of discrepancies and the heterogeneity between studies. However, in our discussion, we have reiterated that the pooled results of the meta-analysis should be interpreted with caution for the reasons you have mentioned.

14. Page 8, Line 24: How did the authors assess the discrepancy rates over time? Did they create time intervals for different studies?

We created time intervals based on dates of payments analyzed and observed no clear trends over time. However, we have elected to remove this from our manuscript as it was not part of the objectives.

15. Page 12, line 33: 33% (confidence interval?)
A 95% CI has been included in this section of the results

16. Page 17, line 43: Should be "Table 4". Also, I believe this is more suited as a figure rather than a table.

We have relabeled this as a figure and changed the numbering.

17. How was a study judged to be of an OVERALL low risk of bias?

We have removed the statement regarding overall risk of bias judgment as bias in one domain is already bias as mentioned by Reviewer 2.

18. The qualitative analysis of "Factors associated with discrepant reporting" is well-presented.

Many thanks!

19. Please provide an adequate explanation for the significant heterogeneity observed.

We have provided an explanation for the heterogeneity observed. We outlined that there were differences between the physician populations and methodologies used for assessment of COI between studies. Most notably, the thresholds required for an article or author to be labelled as discrepant varied between studies. This led to high heterogeneity between studies in the meta-analysis, the results of which should be interpreted with caution.

20. The authors make recommendations for journal editors. However, I believe more recommendations should be addressed for academic institutions as well to educate their researchers on adequate disclosure of COI and put stronger policies to address non-compliance to such guidelines.

We thank the reviewer for their suggestions. We have added recommendations addressing academic institutions as well as guideline development organizations to the “Meaning of the study” section of the discussion. We have also highlighted the necessity of stronger policies to address non-compliance.

21. Also, recommendations for future research are valuable.

We agree with the reviewer. We have highlighted recommendations for future research at the end of the discussion.

22. There are several linguistic and grammatical errors in the manuscript. e.g. Page 3, line 29 "Physicians self-report of financial COI are highly", page 5, line 51 "were defined number and", etc.

We have edited the manuscript in its entirety and addressed these grammatical errors.

VERSION 2 – REVIEW

REVIEWER	Lisa Bero University of Colorado
REVIEW RETURNED	25-Jan-2021

GENERAL COMMENTS	The revised manuscript has been responsive to the peer reviewers and most of my previous concerns have been address. I would still exclude the meta-analyses altogether (they have been moved to the appendix) as inappropriate analysis should not appear even in the appendix. I would definitely eliminate mentioned the "exploratory meta-analysis" in the abstract. The presentation of the pooled data and range is sufficient. It might be useful to have a statistical reviewer assess the inclusion of the meta-analysis.

REVIEWER	Nina Kreuzberger Department I of Internal Medicine, Center for Integrated Oncology Aachen Bonn Cologne Duesseldorf, Faculty of Medicine and University Hospital Cologne, University of Cologne
REVIEW RETURNED	05-Feb-2021

GENERAL COMMENTS	Thank you very much for addressing the issues that were brought up during the previous round of peer review. I have only a few language suggestions for you, you have addressed all major points I had already: p. 4, l. 19: Abstract: usually we write MEDLINE

	p.5 l. 5 … are required to … p.5, l. 15: maybe add that this was not your primary research, but that this is a systematic literature review on this question p. 5, l.23: “Many of the objective data sources used in this study relied on disclosures by industry, which may have inconsistencies.” – many of the objective data sources used in the included studies … p. 5, l. 47: “refers to personal financial benefit, family interest or any special influence or loyalty which could undermine the performance of one’s duty to exercise one’s judgment objectively.” ◊ beginning of quotation missing p.19, l.7 “with academic affiliation were significantly more likely to have undisclosed payments compared than those without” ◊ compared to those without? p. 8, l. 26: Thank you for changing from „qualitative analysis“ to narrative synthesis in the results section. Could you take this over as well for the methods section? (under heading “Data Synthesis”) p. 9, l. 31: “The proportion of funds that was identified as discrepant between self-reporting and objective data was defined as the amount of funding not disclosed as a proportion of the funds recorded in the payment database.” – hard to read p.9, l.40: “A random-effects model was used because of the expected methodological heterogeneity between studies.” – did you expect only methodological heterogeneity, or also heterogeneity in characteristics of included physicians etc.? p.11 l. 11: pool data – pooled data? p. 11, please stay consistent with your terminology and sequence of reporting, above you write: “article, payment, disclosure, and author level”, below the third level you describe is “authorship”. p.17, l.29, “directly, or indirectly, related to the topic of the presentation, clinical practice guidelines, or another publication” – would be interesting to know who evaluated this/ how this was defined, but may exceed the scope of your work. p. 24, l.11: “Data concerning the of association unreported COI and research outcomethe outcome of the research was reported by three studies, “ ◊ data concerning the association of unreported… p. 26, l.24: “be published in favour of studies with low discrepancies. “ – may be more likely to be published than? Appendix: I could not find the flow diagram as appendix, it may have been moved elsewhere in the generated pdf file. Please check numbering of figures, tables and appendices.

REVIEWER	Abdelrahman I. Abushouk Cleveland Clinic Foundation, Cleveland, Ohio
REVIEW RETURNED	19-Jan-2021

GENERAL COMMENTS	The authors have addressed my earlier comments.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 4

1. The authors have addressed my earlier comments.

Thank you.

Reviewer: 1

1. The revised manuscript has been responsive to the peer reviewers and most of my previous concerns have been address.

Thank you.

2. I would still exclude the meta-analyses altogether (they have been moved to the appendix) as inappropriate analysis should not appear even in the appendix. I would definitely eliminate mentioned the "exploratory meta-analysis" in the abstract. The presentation of the pooled data and range is sufficient. It might be useful to have a statistical reviewer assess the inclusion of the meta-analysis.

We thank the reviewer for their insightful comment. We are in agreement that the high heterogeneity precludes the meaningful interpretation of the pooled results. In response, we have refocused our article primarily on the narrative synthesis. We have eliminated mention of the exploratory meta-analysis in the abstract as requested and have modified the title of the manuscript to reflect this. In the main text, we have extensively highlighted that the pooled data should be interpreted with caution throughout the results and discussion as other reviewers have suggested and found acceptable. We have included the meta-analysis in the appendix as we believe some readers would find value in seeing the data despite the high heterogeneity. We hope that this is to the reviewer's satisfaction.

Reviewer: 2

1. Dear authors, Thank you very much for addressing the issues that were brought up during the previous round of peer review. I have only a few language suggestions for you, you have addressed all major points I had already.

Thank you.

2. p. 4, l. 19: Abstract: usually we write MEDLINE

We have capitalized the word MEDLINE.

3. p.5 l. 5 … are required to …

We have added the word “to”.

4. p.5, l. 15: maybe add that this was not your primary research, but that this is a systematic literature review on this question

We have clarified that this study systematically reviewed the literature on this question.

5. p. 5, l.23: “Many of the objective data sources used in this study relied on disclosures by industry, which may have inconsistencies.” – many of the objective data sources used in the included studies …

We have clarified that we are referring to the objective data sources used in the included studies.

6. p. 5, l. 47: “refers to personal financial benefit, family interest or any special influence or loyalty which could undermine the performance of one’s duty to exercise one’s judgment objectively.” beginning of quotation missing

We have removed the incorrect use of quotations.

7. p.19, l.7 “with academic affiliation were significantly more likely to have undisclosed payments compared than those without” compared to those without?

We have corrected the sentence as you suggested.

8. p. 8, l. 26: Thank you for changing from „qualitative analysis“ to narrative synthesis in the results section. Could you take this over as well for the methods section? (under heading “Data Synthesis”)

We have replaced qualitative synthesis with narrative synthesis.

9. p. 9, l. 31: “The proportion of funds that was identified as discrepant between self-reporting and objective data was defined as the amount of funding not disclosed as a proportion of the funds recorded in the payment database.” – hard to read

We have amended the sentence to increase readability.

10. p.9, l.40: “A random-effects model was used because of the expected methodological heterogeneity between studies.” – did you expect only methodological heterogeneity, or also heterogeneity in characteristics of included physicians etc.?

Thank you for your comment. We have added that we expected heterogeneity in the methods and sample characteristics of included studies.

11. p.11 l. 11: pool data – pooled data?

We have corrected this to pooled data.

12. p. 11, please stay consistent with your terminology and sequence of reporting, above you write: “article, payment, disclosure, and author level”, below the third level you describe is “authorship”.

We have revised our terminology to stay consistent by using authorship as our preferred term. This is reflected in Table 1 and in the rest of the manuscript.

13. p.17, l.29, “directly, or indirectly, related to the topic of the presentation, clinical practice guidelines, or another publication” – would be interesting to know who evaluated this/ how this was defined, but may exceed the scope of your work.

We are in agreement with the reviewer that this information would be interesting to know. While in some cases studies report independent, blinded review by a third party to determine relevance,

unfortunately this information was not consistently available across studies and exceeds the scope of the current work.

14. p. 24, l.11: “Data concerning the of association unreported COI and research outcomethe outcome of the research was reported by three studies, “ data concerning the association of unreported…

This sentence has been amended as requested.

15. p. 26, l.24: “be published in favour of studies with low discrepancies. “ – may be more likely to be published than?

This sentence has been amended as requested.

16. Appendix: I could not find the flow diagram as appendix, it may have been moved elsewhere in the generated pdf file. Please check numbering of figures, tables and appendices.

The PRISMA Flow Diagram has been included as Figure 1, which is separate from the appendix. We have reviewed the numbering of figures, tables, and appendices which are correct.